THE COMING OF AGE OF ORAL SEPSIS.

BY

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ON the occasion of which an account appeared in the JOURNAL of May 28th, p. 787, Sir Thomas Barlow kindly mentioned my earlier studies on anaemia (1887–90) and the endeavours underlying them to ascertain causes rather than find remedies. May I refer to the following, which I recall now with interest? When, after five years, I described the results (in 1890), I received a letter from a friend saying that it was all very well to describe results of observations and experiments, but what was the practical good of them? The reply I made was: "I don't know what the practical good is, but these are the facts."

The sequel, ten years later (1900), was the practical one, whose importance is now so widely recognized, to which reference was made by Sir Thomas Barlow—namely, the recognition of "Oral Sepsis" as the greatest septic infection in medicine, and of "Oral Antisepsis" as one of the most important and simplest and easiest measures for the prevention of a widespread group of maladies affecting almost every system of the body.

So far as this great practical measure is concerned, it was infinite curiosity about a piece of pigment of no earthly interest to anyone—followed up by studies and experiments lasting fifteen years—that led me to attach importance to oral sepsis as a disease factor, now recognized to be of the greatest possible practical interest to everyone. This outcome has been the greatest satisfaction of my life.

It is of the more interest to me since, as it happens, this summer "Oral Sepsis" celebrates her coming of age—the 21st anniversary of her appearance in medical literature under her new name. She made her entry very quietly, shyly, and even apologetically, her sponsor introducing her with these words: *

"That the common condition I am now about to describe should have any possible connexion with anaemia, especially with so severe and rare a form of anaemia as the one which (alone among all diseases) bears the deadly prognostic title of 'pernicious,' was something for which I was quite unprepared. But the following are the facts."

Although of good parentage—her parents being Experimental Pathology and Clinical Medicine—she had been and still remained at the age of eleven a very delicate child. To give her courage and individuality I sought for a fitting name, and gave her the name of "Oral Sepsis" ("Oral Sepsis as a Cause of Disease," BRITISH MEDICAL JOURNAL, July 28th, 1900, p. 215). My object in giving this special name was definitely to emphasize the nature and importance of the part she played in connexion with ill health.

"It is not the absence of teeth, but the presence of sepsis; it is not dental defects *per se*, but chronic septic poisoning; it is not defective mastication but effective sepsis; it is not accumulation of fermenting food débris between the teeth, but the presence of virulent streptococcal sepsis in open wounds in the gums and sockets, in the teeth and bone, that underlie the ill health so frequently associated with 'bad mouths.'"

The first reception she got was chilling enough. She was promptly called names—designated a "hypothesis" and a "theory"; even the play in which she was staged—that of "Medical Sepsis," as I termed it in 1904—was said not to exist. I recall, on the other hand, the encouraging note I received from an old friend and good physician, the late Dr. George Gibson of Edinburgh: "You have not only invented a new name, but a new disease." Soon, from 1902 onwards, the interest in oral sepsis grew steadily greater, and it has increased steadily ever since.

If the great subject of sepsis in medicine was a theory so also was the whole subject of surgical sepsis; if oral sepsis was a theory, so also was a fatty tumour. The only difference between the two was that no one knew anything about a fatty tumour except that it was a fact which could not be explained, was not preventable, and did no harm; whereas oral sepsis and the multiple ill effects which it produces were great facts of the first magnitude, which could be explained, and were largely preventable.

Since that time much about "oral sepsis" has appeared in medical literature, and an incalculable amount of oral sepsis has been removed, with incalculable benefit to the world. But an incalculable amount still remains. It still holds true, as summarized in the presidential address (July, 1910) of a physician than whom no one has wider knowledge and experience—Dr. J. Mitchell Bruce:

"Its effects are so widespread, so multiple, and frequently so grave as to make us ashamed of our previous blindness to a common source of blood infection staring us in the face all these years."

But if oral sepsis as a disease factor has thus played, and still continues to play, so important a part, we have now the satisfaction that it is playing it before a big audience; and that many "antibodies" have been formed against it in every branch of medicine, representing almost every system of the body.

To sum up: the gorge created in the landscape of medical diseases by this subject of oral sepsis in the past twenty-one years is both wide and deep; the volume of muddy waters of medical sepsis which has all along been there can now be seen crashing down it visible to all eyes. The rôle of oral sepsis in its causation has received a great volume of recognition, obscured at first by the mists created by its fall, then collecting itself into a great body of facts, till in a period of ten years (1910), and still more now after twenty-one years, it can be seen pouring itself over the "rapids" in a torrent of recognition of its importance not only as a medical but as a great national concern of health.

May I add that I have watched this result with great interest, both from a clinical and a pathological point of view? From the clinical point of view, in the course of some forty years' experience, I can recall nothing so astounding as the marvellous change in health I have seen time and again caused by the removal of obvious, glaring, but continuously overlooked conditions of oral sepsis.

But great as this interest has been, it has almost, if that were possible, been exceeded by the reflection, that so far as my own experience is concerned, it was pathology, and first of all experimental pathology, that formed the basis of my original conclusions. For it was the establishment of the first research endowment in experimental pathology in the country—namely, the John Lucas Walker Studentship in the University of Cambridge—that first gave me in 1887 the opportunity to begin the studies, remote apparently from all spheres of practical interest, that have been associated, directly or indirectly, with such extraordinarily interesting, practical, and unexpected results in the arrest and prevention of disease. So far as these studies have been connected with any of these results, credit may most fittingly be given to the far-sighted and beneficent purposes of those who, like the founder of that Studentship, endeavour to advance the healing art by facilitating investigations into its causation that might otherwise have been impossible.

* "Pernicious Anaemia," *Lancet*, January, 1900.

THE SERUM DIAGNOSIS OF SYPHILIS.

PROFESSOR DREYER'S METHOD.

ON June 2nd Professor GEORGES DREYER, M.D., F.R.S., introduced by Sir ALMROTH WRIGHT, gave a lecture, in the Institute of Pathology and Research at St. Mary's Hospital, on the flocculation test for syphilis, which he has devised.

Professor DREYER began by pointing out that the Wassermann reaction, although for all practical purposes specific, was not really quantitative, so that different workers' results could not be compared numerically, and moreover it was very complicated and employed a large number of variable reagents. In attempting to do away with the necessity for the haemolytic system Herman and Perutz had used the property possessed by syphilitic serum of flocculating a saline suspension of sodium glycocholate and cholesterin. This reaction, however, had proved less sensitive than the Wassermann reaction, and also gave a fair number of false positive results. Sachs and Georgi had improved the flocculation test by using an alcoholic heart extract with cholesterin. To 0.5 c.cm. of this fluid they added 1 c.cm. of the serum to be tested diluted 1 in 10, incubated the tubes for two hours at 37° C., and then left them at room temperature for twenty hours before reading the results. This test also gave in the hands of most workers a certain number of false negative and false positive results.

Professor Dreyer then outlined the ideals to be aimed at in devising a test of this sort. These were: that the technique should be simple; that there should be one